August 22, 1951.

Dr. Tom Nelson,
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Dear Tom:

Thanks for your informative letter of the 10th. Concerning the two-headed enzyme, someone's selling a bill of goods. The Genetics Department does not stoop to low enterprises. Of course we sent the Prize, but not without prior, formal notification to the awardee so that he could rent a tuxedo for the occasion of receiving it.

The succ- story sounds too bewildering now. I'm not sure what you mean by "non-random isolation of progeny" to explain peculiar linkage behavior, unless of course it's just that a restricted subset of recombinations is isolated. Since P and Lac are rather closely linked, you are likely not to get a clear picture of Lac-V linkage when P+ is selected- I trust that is what you mean. But Rothfels has an excellent analysis of the problem of absolute crossover frequencies, though I don't think that a consistent map, including Bl, sm, M, Lac, V, and TL can be drawn, probably because of structural differences,

We hope to see you at Minneapolis. Esther has some rather concrete evidence that lysogenics differ from sensitives by a segregating locus, linked to Gal, as well as (?) the presence of the lambda itself. This would make it something like K/kappa in Paramecium. I don't know whether I've mentioned this before- we've just finished a side-job, a direct proof of the pre-existence of $V_1^r$ and $S^r$ mutations (as illustrative examples), based mostly on a method for enriching for, and isolating, such mutants from cells not themselves exposed to the selective agent.

As to the "story" quoting Vogt, I shouldn't mind at all being beat into print on it. It supposedly refers to K-12, but it sounds extraordinarily like what we think does happen with Salmonella, where there is still no evidence of any sort of linkage or association in the transfer of markers from one strain or mutant to another.

I don't know anything about Vanderbilt except that Ilda McVeigh's there. Delbruck should be able to tell you more.

Sincerely,